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A CRITIQUE OF ACCIDENT
PRONENESS RESEARCH

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November, 1966

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I INTRODUCTION

Man has always been willing to blame fate for spreading chance unequally over his species. However, when the scientific revolution developed, 'fate' was no longer a permissible excuse - everything had to have a 'cause'. It was no longer acceptable to explain a series of mishaps by 'bad luck'. When a piece of scholarly work by two statisticians in 1919 suggested the possible existence of unequal initial liability to have an accident, the belief became widespread that the majority of accidents are 'caused' by 'proneness'. This has remained in much of the literature ever since. That the concept has been unquestioningly accepted by the layman is shown by an illustration of the word 'prone' in 'A Dictionary of Modern English Usage' (Fowler, 1965): "accident prone, a product of the motor-car age". However, in the following review it should become clear that 'proneness' - its detection and treatment - is by no means the answer - not to even a quarter of the accidents. In fact, scientific investigations have since shown that:

- (a) 'Proneness' is no more useful concept than that of 'fate' - no one stable personality characteristic associated with proneness has been found during the last fifty years of research.
- (b) Hence, following (a), no methods have been universally successful in treating prones: each psychiatrist has his own theory.

- (c) Many present-day researchers are very sceptical of the existence, stability, or importance of the phenomenon as evidenced by arguments on theoretical, statistical and/or methodological bases.

The following review is not the first in the field by any means. As far back as 1951 Arbous and Kerrich tried to review the past research of accident proneness in the light of the failure over many years to identify and isolate the phenomenon. Froggatt and Smiley recently (1964) made the most comprehensive review for the British Journal of Industrial Medicine, to quell the undisciplined work of many Industrial Psychologists and medical doctors. The present critique draws heavily on these two excellent reviews, on the original writings in the field, on the recently published text Accident Research (Haddon, Suchman, and Klein, 1964) and on other writers who have made respected contributions to the field.

II ACCIDENT REPEATERS VS. ACCIDENT PRONES

When accident statistics are collated over a period of time for a given population, it becomes rapidly obvious that some individuals have more accidents than others. This led to the belief that one man is more 'prone' to have an accident than another. The following section will attempt to show that this conclusion is not necessarily correct, particularly when drawn from routine accident statistics. Subsequently, the



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reason for the postulation of accident proneness and the methods used to prove its existence, will be presented.

2.1 The Statistics of Equal Liability of an Accident

If in an evening of poker or bridge hands, one player gets one good hand after another, his opponents don't accuse him of being more 'prone' to getting good hands. Given that they trust the honesty of the cards and dealer, they are fundamentally sure that they are all equally liable to get good hands. It is accepted that in a small number of deals such runs of good luck will occur, but as the number of deals increases the 'good luck' will average out. Similarly, even if one man were no more prone to accidents than any other, one year's accident record could indicate that he had several accidents at a rate higher than that of the rest of his fellow workers, and yet, if he lives long enough, his average rate could be the same as the job mean. By elementary statistical methods, the expected number of extreme cases (high accident rates) can be calculated assuming equal liability of the event's occurrence (the accident).

To take the example from the earliest paper in the field of accident research: "Suppose that 100 equally capacious, and equally accessible pigeon holes are bombarded with 20 balls, none of which can fall clear of the pigeon holes altogether, then the chance of any one ball lodging in any particular pigeon hole is one in a hundred, and at the end of the bombard-

ment the distribution of pigeon holes with 0, 1, 2, etc. balls in each is given by the 21 successive terms of (the expansion of):

$$100\left(\frac{99}{100} + \frac{1}{100}\right)^{20} \quad \text{" (Greenwood and Woods, 1919)}$$

Expanded, the terms are shown in the following table:

Number of Balls (N)	Number of Boxes Containing N Balls		Total Number of Balls in Boxes With N Balls
	Term	Number	
0	$100\left(\frac{99}{100}\right)^{20}$	81.66	0
1	$100\left(\frac{99}{100}\right)^{19} \left(\frac{1}{100}\right)^1$	16.46	16.46
2	$100\left(\frac{99}{100}\right)^{18} \left(\frac{1}{100}\right)^2 \frac{20 \cdot 19}{2}$	1.58	3.16
3	$100\left(\frac{99}{100}\right)^{17} \left(\frac{1}{100}\right)^3 \frac{20 \cdot 19 \cdot 18}{3 \cdot 2}$	0.097	0.29

Table 1: Expected Distribution of 20 Balls Among 100 Boxes, all Equally Accessible.

Thus if 20 balls are dropped into 100 boxes, 10 consecutive times, one can expect that during one time one box will have 3 balls in. This is an unlikely event but has a probability of about one in ten of occurring, and is just as likely to occur on the first try as on any. Similarly, even if accidents were as randomly falling on people as the falling of a ball in a box, a few people (boxes) can be expected to have an unusually large

number of accidents (balls).

In the example, the total number of balls and boxes were predetermined; in the problem of accidents and people, the latter number is determined by the group size under consideration, but the number of accidents changes with time. We require the probability of a man having 0,1, 2, ... accidents in a given time. These probabilities are now expressed by the terms of the (Poisson) series:

$$e^{-x}(1 + x + \frac{x^2}{2} + \frac{x^3}{6} \dots)$$

Where x is the average number of accidents / unit time. Thus if we assume that a group of 100 men have 20 accidents per month (taking the numbers from the 'boxes and balls' example) how many men do you expect to have 3 accidents in a month, assuming that no one is any more predisposed to have accidents than any one else? The average number of accidents per month (x) is 20/100 or 0.2, and the expected distribution would be:

Number of Accidents (N)	Number of Men Suffering N Accidents		Total Number of Accidents
	Term	Number	
0	$100e^{-x}$	81.67	0
1	$100e^{-x}x$	16.37	16.37
2	$100e^{-x}x^2/2$	1.64	3.28
3	$100e^{-x}x^3/3.2$	0.11	0.33

Table 2: Expected Distribution of 20 Accidents Among 100 Men, All Equally Likely to Suffer an Accident.

Thus if you take 10 groups of 100 men each, you expect to find at least one man having 3 accidents in a month, i.e. one in a thousand will have 3 accidents during the month, even though every man was equally likely to have an accident.

As an aside, the similarity between the results of the examples of the 'boxes and balls' and the 'men and accidents' is obvious. The modification due to the time dependence of accidents only slightly alters the statistics.

It cannot be emphasised too strongly that even if everyone is equally likely (or unlikely!) to have an accident, pure random chance leads us to expect some people with many accidents.

2.2 Two Hypothetical Industrial Accident Distributions

Suppose, we take a hypothetical example of a group of 100,000 workers having 15,000 accidents per year (this is typical of compensatable accident claims in heavy industry) and we assume as a start that the men are all equally likely to suffer an accident (a highly unlikely assumption even if only due to different jobs within the industry). The accident rate $x = \frac{15,000}{100,000} = 0.15$ per year. The distribution of accidents expected would be as shown in Table 3.

Number of Accidents (N)	Number of Men Suffering N Accidents	Total Number of Accidents
0	86,080	0
1	12,910	12,910
2	968	1,936
3	48.4	145
4	1.8	7.2
	0.05% 1.02% 14%	1.0% 14% 100%

Table 3: Expected Distribution of 15,000 Accidents
Among 100,000 Men, All Equally Liable.

Number of Accidents (N)	Number of Men Suffering N Accidents	Total Number of Accidents
0	266	0
1	368	368
2	256	512
3	119	357
4	42	168
5	6	30
6, or more	3	35
	16% 75%	40% 100%

Table 4: Expected Distribution of 1,470 Accidents Among
1,060 Individuals, All Equally Liable.

Thus we would expect, on pure chance, about 2 men to have 4 compensatable accidents in a year in the population of 100,000 and more than 1,000 will have more than one accident.

Similarly, a second example to illustrate the expected distribution of minor injuries, was taken from Froggatt and Smiley (1964). An actual case of 1,060 individuals suffering 1,470 minor accidents over a period of time was examined. Assuming pure chance the expected distribution is shown in Table 4. Notice that 51 men are expected to have 4 or more accidents despite the fact that the 'average' man has only 1.4 accidents.

Some people in both examples will appear on hind-sight to be 'accident repeaters', which is true: they have had repeated accidents. However, in these examples it is clear that this is through no personality-situational fault of their own. They are no more 'prone' to accidents than any other man, since their probability of having an accident tomorrow is still exactly equal to that of all the men in the group.

2.3 Fallacy of Quoting Percentage Figures

Also from such hypothetical cases the fallacy of quoting percentage figures to dramatise the existence of 'prones' becomes clear. Typical examples of this type of propaganda are:

(1) In a paper in the Journal of Industrial Medicine a psychiatrist quotes "In Detroit industries it is estimated that

12% to 15% of the employees have close to 100% of the plant accidents", (Moorad, 1947). The author wished to indicate that since only a small percentage of the work population had most of the accidents, i.e. the 'average' Joe did not have one, the author is "forced to conclude" that these people must be different from the 'average', i.e. 'prone'. On closer scrutiny this reasoning is quite fallacious: clearly if there are 14 accidents among 100 men, only 14% of the men, at the most, will have 100% of the accidents! Such quotations appear to indicate that the author would be happier if 100% of the men had 100% of the accidents (i.e. increase the accident rate seven-fold), then he would not be "forced to conclude" that some are more prone than others.

(2) In an accident prevention advisory letter to a company it is stated "4.6% of the total employees accounted for 42% of the serious accidents in the company", (private communication). To show that this does not demonstrate the presence of 'prones', we return to the hypothetical accident distributions above. In the first, 1% of the men, all equally likely to have an accident, have 14% of the accidents and 7% of those who have an accident have 2 or more. This 7% cannot be 'accident prone' even though they are 'accident repeaters', since they were assumed to have no more chance than any others. In the second example, 16% of the men had 40% of all the accidents and 1% of the men are expected to have 5 or more accidents without being accident prone. In

fact, for the case in the advisory letter, from the knowledge of the total number of accidents in that company, it was calculated that 5.6% of the men will be expected on pure chance distribution, to have 42% of these accidents.

The above two quotations of percentage figures indicate how misleading they can be. The first example is rather ridiculous, but the second is more subtle, causing many accident investigators, collators, and researchers to embrace so blindly the concept of accident proneness to explain the incidence of accident repeaters.

III INITIAL DEVELOPMENT OF THE CONCEPT OF ACCIDENT PRONENESS

Having indicated that accident repeaters are expected in a group of men all equally likely to have an accident, we should investigate the reason for the development of the concept of accident proneness.

Essentially, in 1919 Greenwood and Woods collected accident data from 14 groups of 50 to 750 women; a typical accident distribution was that for a group of 648 women munition workers over five weeks:

Number of Accidents (N)	Number of Women with N Accidents	Expected Accident Frequency When Distributed By:		
		Chance (Poisson)	Single- Biassed	Unequal Liabilities
0	448	406	452	442
1	132	189	117	140
2	42	45	56	45
3	21	7	18	14
4	3	1	4	5
5	2	0.1	1	2

Table 5: Comparison of Three Hypothetical Frequency Distributions With a Measured Distribution of 311 Accidents Suffered by 648 Women Munition Workers (Abridged from Greenwood and Yule, 1920)

The distribution of accidents among the women, predicted by assuming them all equally likely to have an accident, does not fit the collected data accurately. In statistical terms it can be shown that the two distributions are "significantly different" i.e. the difference between the two distributions is unlikely to appear in a sample of this size by chance (recall, the run of good hands is unlikely to continue for many deals; if it does, it is likely that the cards are not random). Hence we assume they are, in fact not the same - i.e. the existing distribution is not a pure random chance distribution. Thus the statisticians looked for other methods of prediction.

One hypothesis was that all the women started equal but upon having an accident, their chance of having another increased.

This is the distribution labelled "Single-Biassed". The latter predicted distribution fits the measured distribution quite closely. However two objections to this distribution arose:

(a) It did not fit all 14 sets of data as well as expected.

Also, Miss Newbold in 1926 found the fit of this distribution to be poor for all 15 groups of over 100 men it was tried on.

(b) The concept of becoming more liable to have another accident after a first does not agree with psychological knowledge about learning. This is, in effect, "negative learning" (Jacobs, 1956).

A third hypothesis was that the worker starts with unequal chance of having an accident (Be wary - this is the source of proneness!). This too fits the measured distribution better than the pure chance prediction, and in this example is an acceptable hypothesis. Greenwood and Woods concluded "It appears just to infer that the hypothesis of the deviation from simple chance being dependent upon unequal initial liability is sustained". Newbold (1926) found "the unequal liability theory is in all cases a great improvement on the Poisson (pure chance). It is still far from a very good fit in some cases but ... the results on the whole are fair".

Nowhere in these two papers do the authors claim to have proved the existence of personal proneness. The first paper

remarks: "results indicate that varying individual susceptibility to 'accident' is an extremely important factor in determining the distribution, ... (and) ... it seems that the genesis of multiple accidents under uniform external conditions is an affair of personality." But, as Haddon, Suchman and Klein (1964) pointed out "this is a diagnosis of exclusion reached in the absence of any study of personalities." Greenwood and Woods did however caution, "if this conclusion be confirmed by a wider investigation ...". Newbold's paper, which attempted to make this confirmation is more reserved in concluding "it is not possible in a mass examination of this kind to find how much of this (difference of the distribution from pure chance) may be due to individual differences in the conditions of work, or how much to personal tendency, but there are many indications that some part, at any rate, is due to personal tendency."

IV SUBSEQUENT DEVELOPMENT

"Despite the fact that these studies above were made about a quarter of a century ago, they still must be regarded as almost complete summaries of our existing knowledge of this phenomenon, as little real advance has been made since that time" (Arbous and Kerrich, 1953). Thirteen years later this statement is still true, though many researchers in the field now question the importance of the accident proneness phenomenon as an isolated source of accidents. To examine the phenomenon more closely, the concept was defined formally and subsequently research attempted

(a) to identify the individual with above average 'proneness' by laboratory and clinical psychological tests and, (b) new statistics were collected to reappraise the original foundation.

4.1 Definition and Clarification of the Concept of Accident Proneness

Following Newbold's tentative confirmation, Farmer and Chambers (1926) extravagantly claimed that "previous statistical investigations have shown that industrial workers exposed to equal risks were unequal in their liability to sustain an accident and that this unequal liability was a relatively stable phenomenon". The term 'accident proneness' was coined about this time and they made their definitions of the terms clear: "'Accident-proneness' is a narrower term than 'accident-liability' and means a personal idiosyncrasy predisposing the individual who possesses it to a marked degree to a relatively high accident rate.... We regard 'accident-proneness' as an individual quality of relative permanence, and use 'accident-liability' as the sum total of all the factors determining accident causation." In summary, proneness was defined as meaning: a personal idiosyncrasy of relative permanence predisposing the individual to a higher rate of accidents. This definition is the crux of all the research in this field. If any one or other of the defining elements are missing from work propounding proneness, then the 'proof' is meaningless to the case.

It might be instructive to illustrate this definition by presenting variables which cannot be the 'cause' of proneness. Proneness is concerned with permanent personality aberrations such as above or below average intelligence, social behaviour, self assertion etc., it is not concerned with temporary aberrations of this nature, nor is it related to physical descriptors such as age, performance, health etc. The following two examples discuss a personal descriptor and an environmental condition which are known to accompany high accident rates but which cannot be considered factors of proneness since they do not fit the above definition.

The most significant finding in all accident research to date is that age has considerable relationship to the accident rate. This has been frequently reconfirmed since Newbold (1926). The young and the old have the highest rates. Experience or inexperience is apparently also a factor since some studies indicate that the rate is highest in the first few months on the job (e.g. Humke, 1936), but on removing this variable statistically, the experienced young still have higher rates than the average in the same risk condition. A young person is not more prone to accidents since his youth is not a permanent condition, c.f. above definition, but he is more liable. The automobile insurance companies are well aware of this fact, and since it is their business not to lose money, they increase the premiums for the young.

An environmental condition which relates to accident rates is temperature. It has been found (Osborne, Vernon and Muscio, 1922) that there is an optimum working temperature (67.5°F) above and below which the accident rate increases. Other studies have confirmed the existence of a minimum, but the temperature at which this minimum occurs is quoted anywhere from 59° to 69°F. A person working in cool or hot temperatures is obviously not more prone since temperature is not a personality attribute but he is more liable to have an accident.

There are many other factors which have been shown to have a relationship with accident liability: health, vision, alcohol, fatigue, mutual attitudes between plant management and the worker, family worries, absenteeism, poor machine design (poorly guarded and/or 'human engineered') etc. Some of these variables are still subject to criticism by researchers and contradictory findings exist. Frequently the difficulty of 'cause and effect' arises since any one of these variables could be symptoms of another, resulting in the appearance of spurious causes of accidents. A typical example of this was the recent finding (Adams, 1965) that smokers have more traffic accidents; it is probable that smoking does not cause accidents but rather that both are the results of some other condition, hypothesised by the authors as "over-dependency".

The above cursory discussion of typical accident research findings has attempted to clarify what is not accident proneness,

and in the following discussions the definition of the concept should be uppermost in the mind since most of the critiques are essentially attacking this definition.

4.2 Laboratory Studies to Predict 'Prone's'

Greenwood and Woods first pointed out that if an individual's personality was such as to permanently affect his accident liability, "by weeding out susceptibles the accident rate would necessarily decline." Farmer and Chambers' studies in 1926 mark the beginning of a long line of effort to devise psychological laboratory tests which would accurately predict the 'prone's'. In this first attempt the authors doubted the validity of some of their tests (intelligence and 'perseveration') and were not dismayed to find they had no predictive value. Only their 'aesthokinetic' tests (e.g. coordination, reaction time), which can hardly be considered a measure of 'personal idiosyncrasy', gave any indication of success, predicting reasonably for one group out of the five tested. Since their conclusions were rather more optimistic than their experiments warranted, work in the field flourished. "As more and more personality tests were used in subsequent research (Farmer and Chambers, 1929, 1933, 1939; Slocombe and Brakeman, 1930; Marbe, 1935; Vernon, 1937; Korngold and Lahy, 1937; Desilva, 1939) the contradictory and confusing findings that resulted brought the accident proneness concept... into disrepute among informed

research workers, but not among the public" (Haddon, Suchman, and Klein, 1964). This continued throughout the 1930's.

When the first serious attempt was made in 1941 by Chambers and Yule to re-confirm the statistical evidence found by Newbold fifteen years earlier, the authors suggested a modification of the concept, i.e. that 'proneness' varied with time. If true, this naturally makes any prediction of 'prones' virtually impossible since prediction depends on the stability of the phenomenon. The 'disrepute' of this personality-testing technique is amply confirmed by the fact that only one report since 1939 has applied similar tests: Whitfield (1954) made a serious attempt to locate significant personal and personality factors to predict 'proneness' which he found in a group of 1,384 miners. The only useful conclusion re 'proneness' that he could make, was that 'psycho-physiological tests' were reasonably accurate predictors for young men, and 'motor control' tests for older men, (both being physical, rather than personality descriptors). Thus, he found no universal test giving the accuracy which would be required for administrative separation of 'prones'. However Vernon was not discouraged since in 1945 he remarked that psychological tests for detecting proneness should be used at the time of job selection. Also, Chambers in 1955 still maintained the usefulness of such tests to predict proneness to accidents in complex situations, but felt that the tests were not adequate for simple task accident prediction.

Previous tests were searching for personality traits unrelated to the accident situation. Recent developments have been to attempt to locate behavioural differences among repeaters in risk or hazard situations (Rockwell et al. 1961). Their experiments still continue and any conclusions they draw will have to be verified by other workers. However, despite other interesting results, at present no correlation with past accidents and risk taking behaviour has been found.

From the presentation of results in this section it is concluded that up to the present the laboratory personality tests have not been a success, and lead to no absolute method of predicting a 'prone' individual.

4.3 Clinical Studies

Doctors and psychologists, particularly those in industry, seemed to have avidly accepted the concept of accident proneness as an innate and stable characteristic, presumably because it gave them something concrete to search for, while treating many accident victims. Throughout the years since 1930, publication of psychiatric patterns observed in accident prones have been appearing. Despite their many methodological shortcomings, some patterns, though often contradictory, are evolving.

The shortcomings of most of the psychiatric studies are:

- (a) Seldom are non-accident repeaters searched for similar psychiatric personalities, i.e. there is no indication that

conclusions drawn for 'prones' are only true of this group.

- (b) Generally there are too few subjects to adequately represent the population.
- (c) The data collection is very subjective and conclusions are opinions on trends formed over a long period of time.
- (d) No attempt to study subjects with equal risk exposure is made.
- (e) Any a priori knowledge of the subject's accident history can bias the conclusions since "it would be an unimaginative psychologist or psychiatrist who, after an accident had occurred, could not find some indication of the conflicts and repressed feeling"...(McFarland, 1957).
- (f) Studies made after the accident occurred could be observing the accident's effect on the personality, especially if authorities have been involved, rather than the personality's effect on the accident as desired.

The terms appearing most frequently to describe the accident repeater are "poor social background" (family disruption or anxieties in past and present), "socially maladjusted" (as shown by public records), "emotionally unstable", "hyperactive" (in children), "suppressed aggression" (in adults), "insecure", "frustrated", "aggressive" (in children). Some psychiatrists make extravagant claims with no supporting data e.g. "It is

estimated that...28% of the total employees have sufficient personality and emotional disorders to be potentially detrimental to efficient and safe work" (Moorad, 1947), or "every male worker showed that emotional tone exhibits recurrent fluctuations of 5-6 weeks... and more than half the accidents seen took place when the worker was in some 'low' emotional state" (Hersey, 1936).

Other more thoughtful and analytic studies admit that there are a multitude of personality descriptions for accident repeaters, frequently contradictory and no two the same. Recently Schuster and Guilford (1964) in order to discriminate usefully between 'problem drivers' and controls, found it necessary to use a 395 item personality inventory.

One important and more concrete result, which has been confirmed by McFarland and Moseley (1954), appeared in Canadian automobile accident research (Tillmann and Hobbs, 1949). Comparison of the frequencies of various kinds of antisocial behaviour or inadequate personal adjustments (as measured by records of welfare, courts, collection agencies etc.) between accident repeaters and accident free individuals, indicated that the former are socially more maladapted and the phrase "A man drives as he lives", was coined. Here is a possible 'predictor' but the usefulness is questionable. The technique may well predict '85% of the repeaters' in McFarland's study, but how many non-repeaters were incorrectly predicted? For example, in the extreme case, if we predict that every member of a group will be

a repeater we are sure to have predicted all those (100%) that had repeated accidents, but we have been 100% incorrect about those who didn't repeat.

Once more we must conclude, that at the present state-of-the art "there is some doubt, even in psychiatric circles, that there exists an identifiable type which could be labeled an 'accident prone personality'", (McFarland, 1965). This is not to deny the usefulness of the clinical approach on the individual basis. If a clinical psychologist is able to identify some personality disorder which could apparently increase the patient's likelihood of incurring an accident, he is doing the patient, as an individual, a humanitarian service by treatment. However, as discussed above, little can be done in this approach to reduce the overall accident rate. A relevant quote is from Accident Research (Haddon, Suchman and Klein, 1963): "A related and particularly dangerous misuse of psychological approaches is to justify in a logically circular fashion administrative actions against individuals who, for whatever reason, have been in repeated accidents. In the present state of our knowledge, we cannot reliably attribute an individual's accident history to his psychological characteristics and hence there are no grounds for basing administrative action upon psychological theory, or for attempting to validate psychological hypotheses on the basis of administratively accumulated accident histories".

4.4 Statistical Studies and Criticisms

4.4.1 The Stability of Proneness

As mentioned in the discussion of the laboratory studies, the first attempt to reconfirm the tentative conclusions reached in 1919 and 1926 was not made until 1941 by Chambers and Yule. After these experiments Chambers retreated from his original definition by suggesting that proneness still existed but varied with time.

Horn (1947) studied pilots with repeated accidents and showed that 15% had their second accident within a month of the first, from which, he concluded, the occurrence of one accident alters the probability of having another. This is rather like the second hypothesis of Greenwood and Woods. However, Mintz (1956) demonstrated that this distribution of accidents in time would be expected even if the accidents occurred at quite random time intervals, i.e. the 'bunching' of repeated accidents is to be expected in a population of equal liability. Thus Horn's work only served to confuse the issue, since no proneness at all, permanent or temporary was required to predict his observed distribution.

The apparent instability of accident proneness was cause for reappraisal of the concept, since no form of prediction can be useful if the subject's proneness level varies from one time to another. An example of the apparent uselessness of prediction of 'prones' is taken from Adelstein's (1952) accident data on 104 shunters over three years. He reports the

effect of removing the records of 10% of the men with the highest accident rates in the first year from the data of the following two years.

	1st Year	2nd Year	3rd Year
Mean accident rate of 104 men	.557	.355	.317
Mean accident rate of 94 men remaining after removal of 10 'worst' in 1st Year.	.393	.361	.329

Table 6: Accident Rates For Shunters Who Joined in 1944 and Shunted For Three Years (Adelstein, 1952)

In this case the annual accident rate actually went up a trifle when the first year's 'worst' ten men were hypothetically removed.

The reappraisal appeared at the end of the forties when several learned critiques of the proneness concept were published (Mintz and Blum, 1949; Greenwood, 1950; Maritz, 1950; Adelstein, 1952; Arbous and Kerrich, 1951, 1953). All attempted to reexamine the basis for the concept, and to indicate the misconceptions that had become so undisciplined.

4.4.2 Theoretical Criticisms

The most important criticisms were on theoretical grounds. It was shown that finding a fit between Unequal Liability and the data does not necessarily prove that this is the explanation for the data. In fact a number of alternative hypotheses yield exactly the same theoretical distribution which could also be explanations. Froggatt and Smiley (1964) presented the following

discussion of this point: "When evolving his hypothesis Greenwood had to assume equal risk to accident and an unchanging environment among his group members, but if in fact some individuals in an experimental population were more (or less) exposed to risk than their colleagues, or if the environment changed in an unequal fashion for the group members over the period of study, then the Distribution of Unequal Liabilities could arise, when in fact accidents were randomly distributed among the subgroups of true equal-risk exposure. (Or again) ... if the probability of sustaining an accident changes after an accident has been incurred the Distribution of Unequal Liabilities may again arise if the law of change for the probability is suitably chosen (Irwin, 1941; Arbous and Kerrich, 1951; Jacobs, 1956). And there are other alternative hypotheses which produce the same distribution, some being coherent in the accident situation (Irwin, 1941) and some derived for other purposes absurd (e.g. Thyron, 1960)" (Froggatt and Smiley, 1964).

The other aspect of the statistics receiving critical attention is the method of data collection. Above, it was indicated that such conclusions tacitly assume that the environmental risk is the same for all workers within the group, i.e. that the only difference between the workers on their jobs is their personality. The Unequal Liability distribution only assumes that the chance of having an accident varies among the group and cannot discriminate whether this is due to personal and/or

environmental variation. It is virtually impossible to find a group of equal environmental risk exposure. At first glance a group of men all sorting at a belt have an identical environment but careful investigation could show any number of variations e.g. lighting, heating, draughts or noise may not all be uniform, and even such aspects as space-to-manoeuver within may vary. In more hazardous tasks lack of equal risk exposure becomes more obvious. Whitfield (1954) attempted to correct for unequal exposure in a coal mine but naturally could not account for all possible variables. A number of studies of accident proneness have been in the traffic field (e.g. Brown and Ghiselli, 1948; Slocombe and Bingham, 1927; Willet, 1964; Slocombe and Brakeman, 1930; Cleveland, 1959) and here the mileage, travel time, locale, time of day, etc. all vary the risk exposure. Schuster and Guilford (1964) removed the effect of mileage only, while Cresswell and Froggatt (1963) attempted to remove a number of such variables. Since the search is for the 'cause' of a rather small difference between the pure chance and the measured distribution, which can be due to either unequal individual susceptibility or unequal exposure to risk, any appearance of the latter can hide the existence of the former.

Other biases in statistical results can be due to inherent biases in the original source of accident data. To collect accident data, one must define the accident. The excellent review of the field, Accident Research (Haddon, Suchman and Klein, 1964) summarises the many definitions : "An accident is defined,

explicitly or implicitly by the unexpected occurrence of physical or chemical damage to an animate or inanimate structure."

The problem arises in accident data collection of what constitutes 'damage': a small cut self-treated with a Band-Aid, a visit to the factory clinic, referral for external medical treatment, absence from work for more than a specified time, reporting to the accident insurers or compensation board, settlement by the accident insurers or compensation board, or finally, death? All these definitions and others have been used by statisticians collecting accident data, and yet the relationships between the definitions have no proven significance. All introduce their own inherent bias into the data which is unrelated to the cause of the accident. Clinic reports were recognised in the earliest work to be a measure of the "tendency to report accidents" rather than the "tendency to have accidents", thus any personality descriptors found from such data may well describe why the man reports rather than why he has accidents. Using fatality statistics includes the man's ability to survive the damage. Some workers have tried to use observed accidents and 'near misses' (e.g. Satterwhite and Laforge, 1966) but the difficulties are obvious.

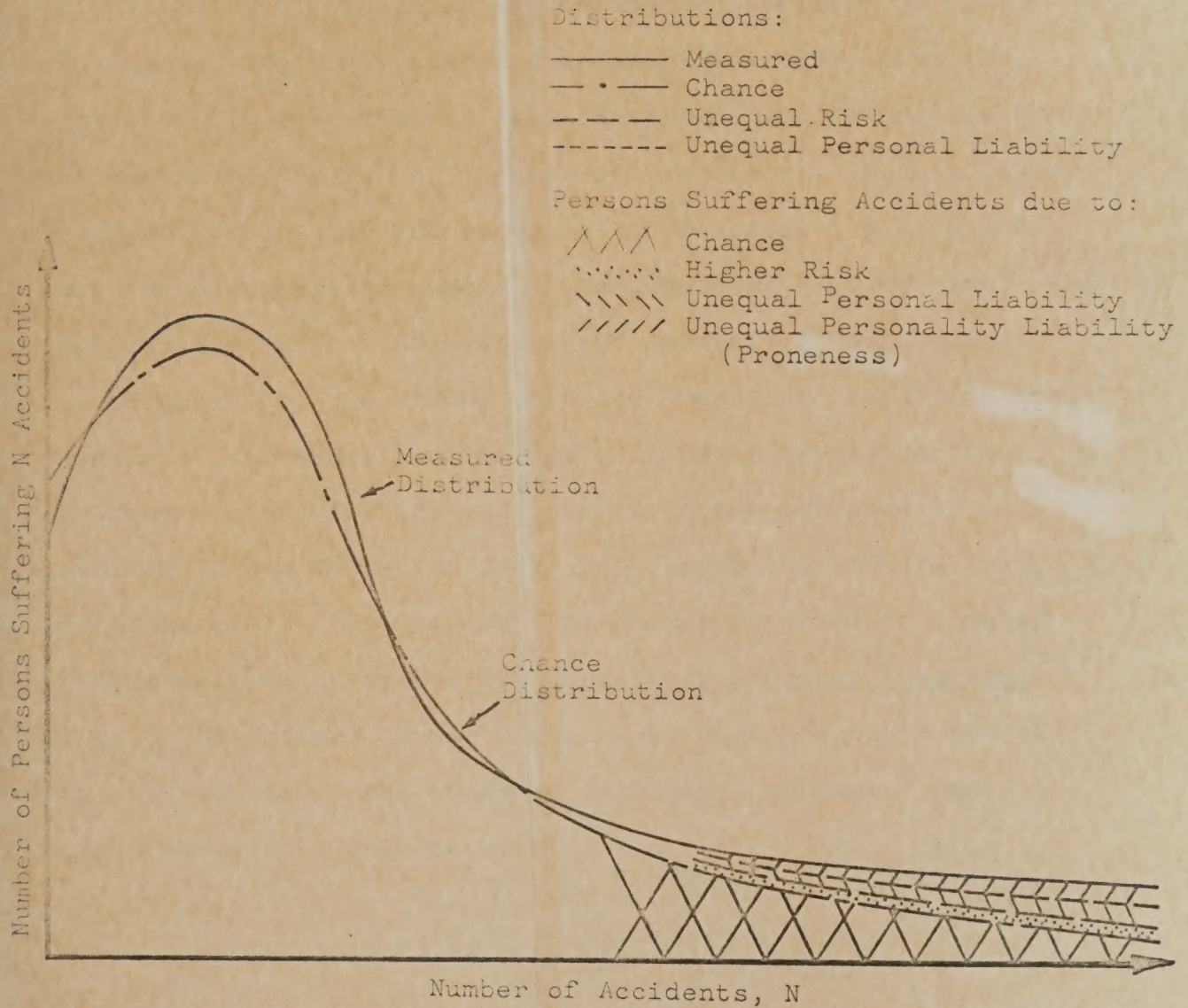
This section has attempted to present the theoretical criticisms against the statistical methods used by many researchers in the first half of the twentieth century, which could seriously bias and possibly invalidate their results.

Doubt has been thrown on a vast majority of the conclusions drawn, except those of the original workers in the field, who couched their conclusions in very cautious terms.

V SUMMARY OF PRESENT THOUGHT

There are three essential factors in the present approach to the concept of accident proneness:

- (a) It is extremely difficult, as witness forty years of attempts, to isolate such a phenomenon from routine accident statistics.
- (b) If it exists, it may vary with both time and situation for any one individual, quite apart from variations due to the measured increase in liability of the young and old, for the inexperienced, for the drugged, or for the inebriated, etc. This variability would necessarily make prediction both difficult and of doubtful value.
- (c) 'Prones', whether they exist or not, at most represent a very small number of the repeaters. They are a fourth order term: The vast majority are victims of 'chance'; some suffer higher risk exposure due to the environment; a few represent unequal personal liability, e.g. the very young or old, the inexperienced, etc.; and the 'prones' would be the smallest term of all, (see figure).



Schematic Drawing to Show a Hypothetical Distribution of Accidents and to Compare With Three Theoretical Distributions

The conclusion should be a plea to workers in this field to avoid using the term 'accident proneness': the terms 'liability' and 'proneness' as defined so strictly by Farmer and Chambers in 1926, are used indiscriminantly. Also the apparent time variation of 'proneness' has led to the use of the contradictory phrase 'temporary proneness'. Norman (1962) proposed using 'accident repeater' to describe a person who has more than a given number of accidents in a given time. This phrase excludes the inflexible term 'prone', is noncommittal about causation, and allows that the 'repeater' condition could fluctuate. Cresswell and Froggatt (1963) suggest 'variable accident tendency' which indicates that the personality is involved in the causation claim but avoids the inflexibility of 'proneness'. But Froggatt and Smiley remarked on closing their excellent review of The Concept of Accident Proneness (1964), "the wisest course might be to discourage the use of any catch phrase: the abuse of 'accident proneness' must always serve as a grim example".

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